ding the nature of Chalk, or of any more ancient Foraminiferal limestone, by the condition of its altered forms; the evidence of their Organic origin being supplied by the microscopic examination of specimens exhibiting the least evidence of change, and this evidence being not in any degree invalidated by the most complete mineralisation of particular examples. A large number of specimens of Ophicalcite have been submitted to me from various sources, as to some of which I have been able to say pretty confidently that they were originally Eozoic, but have been altered by subsequent metamorphism; whilst others do not present any feature whatever which would lead me to assign to them an Eozoic origin. To assume that these last (of which the Strath rock may be an example) are to be placed in the same category with the Canadian Eozoön, and thence to affirm that because they are purely Mineral productions, *it* cannot be Organic, involves a *petitio principii* by which it would be perfectly easy to prove the same thing of Chalk.—Let me illustrate my position by a parallel case. I have lately demonstrated[*] the existence of a Foraminiferal structure *departing much more widely than Eozoön does from any previously known type*, in a class of globular bodies from one to two and a half inches in diameter, occurring in the Upper Greensand; these having been previously regarded by experienced Geologists as mere Mineral concretions. Now, it so happens that the Magnesian Limestone of the North of England contains large masses of spherical concretionary bodies, bearing a strong *general* resemblance to *Parkeria* in internal structure as well as in outward form, but hitherto regarded, I believe universally, as Inorganic; and a reasoner like Mr. Reade would argue in this way:—"Because impartial geologists have pronounced the Permian concretions undoubtedly inorganic, the Greensand spheres are so likewise; and Dr. Carpenter's *Parkeria* becomes extinct as a fossil." But it likewise happens that the structural features which are most peculiar in *Parkeria* present themselves also in a remarkable living Foraminifer recently obtained from great depths in the *Porcupine* dredgings; so that the truly Foraminiferal nature of *Parkeria* cannot be a matter of the slightest doubt. And the only question now is, whether a careful microscopic examination of the minute structure of the Permian concretions may not afford, through its likeness to that of *Parkeria*, more or less definite indications of *their* Organic origin, obscured by subsequent metamorphism. The application is obvious.

6. I am equally unable to admit that if a rock presenting all the characteristic features of the Canadian Eozoön were to be found shading off into one containing characteristic Liassic fossils, this would afford the least tittle of evidence against the Organic character of the former. As the *Lingula* of the ancient Siluria has, in the judgment of our most eminent Brachiopodist, come down unchanged to the present time, and as even the same *varietal* modifications of Foraminiferal types were existing in the Triassic period as now in the Mediterranean, I see no reason why we should limit Eozoön to the Laurentian epoch. When this subject was last discussed at the Geological Society, I ventured to say that it would not in the least surprise me to find Eozoön, or something very like it, now existing on the deep-sea-bottom; and the notion was not treated by any of the eminent Geologists then present as having any *a priori* improbability. Since that time, the Coccoliths first discovered by Prof. Huxley, and the Coccospheres first observed by Dr. Wallich, in the Globigerina-mud, and afterwards recognised by Mr. Sorby in Chalk, have been detected by Prof. Gümbel in Silurian rocks; so that it is clear that the Biological condition of the deep sea has changed much less in vast periods of Geological time, than has that of shallower waters; whilst the probability has now almost reached a certainty that Rhizopodic life has been at least as largely concerned in the production of Calcareous deposits in earlier Geological periods, as we know it to have been in the later.

7. Though Mr. Reade "feels assured that whenever impartial Geologists take the question up, the fossil itself will become extinct," his assurance is not borne out by the judgment of the large number of impartial Zoolgists, Continental as well as British, who have satisfied themselves, by a careful examination of my series of microscopic preparations, of the Organic nature of the Canadian Eozoön, and have authorised me to express their entire accordance in my interpretation of its phenomena. An eminent Professor in one of our own Universities used this emphatic expression—"The matter seems to me not to admit of *hesitation*, much less of *doubt*." My last Continental visitor, Prof. Carus, who is well known to possess a comprehensive and practical knowledge alike of Zoology, Palæontology, and Mineralogy—assured me that having come without any prepossession on the subject, he left me with a full conviction of the justice of my views. The respect paid by such Naturalists as Professors Milne-Edwards, Carus, Lovén, Van Beneden, and Escher von der Linth,—typical representatives of the Science of France, Germany, Sweden, Belgium, and Switzerland,—to my own judgment in a matter as to which *they* regard the special studies of a third of a century as giving me some claim to authority, may console me for the contemptuous repudiation of microscopic evidence in which Mr. T. Mellard Reade has thought it becoming to indulge. I am far from expecting, however, that anyone should pin his faith upon my own *ipse dixit*, supported though it be by the entire concurrence of my three fellow labourers in Foraminiferal investigation, Messrs. Parker, Rupert Jones, and H. B. Brady. And if it be thought that the decision of any tribunal of really "impartial geologists" is likely to carry more weight with the scientific public than that of the authorities I have cited, I am perfectly willing to go into the question with them; provided, however, that such tribunal consists of, or at any rate includes, men who are sufficiently conversant with the Microscopic appearances of undoubtedly Organic structures, to be able to recognise such appearances when they see them. One of the strongest opponents of the Organic origin of Eozoön designated as "an agatized mineral" a section of a *recent* Nummuline shell, that exhibited a minute tubulation corresponding with that of the nummuline layer of Eozoön, which he had just before characterised in the same manner. Another attributed the production of a perfectly mineralised internal cast of *Polystomella* in green silicate, from Capt. Spratt's Ægean dredgings, to the working-in of mud. And a third has abstained from even looking at my specimens, though I have repeatedly expressed my willingness to give him an opportunity of examining them. Such are *not* the judges before whom I would consent to plead the cause of *Eozoön*.

WILLIAM B. CARPENTER

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[*] See my description of *Parkeria* in the Phil. Trans. for 1869.

Mimicry *versus* Hybridisation

ALLOW me space for a word or two in reply to Mr. Wallace and Mr. Butler's observations on my papers on Mimicry and Hybridisation.

There is only one point in my argument to which they have taken exception, and although, of course, I am not therefore entitled to assume that their silence on other points means assent, I may at least infer that in their view the point objected to is most open to assault, and that if it were established, the reader may regard the rest with increased confidence.

The objection is that the instances of hybridisation in plants which I have cited as parallel to the cases of mimicry between the Danaids and Nymphalids were merely cases of hybridisation between species of the same genus or allied genera, whereas these butterflies are more distantly related. The question, as thus put by these gentlemen, resolves itself into a question of comparative degrees of affinity, and Mr. Wallace, with his usual skill, tries to throw the onus of proof from his shoulders to mine. But with all submission we shall keep it where it naturally lies. He puts it that my argument rests on the assumption that hybridisation can take place between different orders or families, and quite logically (supposing me to have done so) objects to my making any such assumption in regard to insects, seeing that nothing of the kind has ever been observed in other animals or in plants. But I rest my argument on no such assumption. I ask no other measure for insects than is given to plants. It is Mr. Wallace who makes the assumption that the amount of difference between Lepidoptera has a different value from that attached to it in any other organic beings. It is he who claims for differences which in any other creatures would be regarded as no more than specific the importance of generic or ordinal. But however this may suit the artificial classification of the systematist, we cannot allow it when we come to deal with the actual workings of nature.

I am not surprised that either Mr. Wallace or Mr. Butler should take what appears to me an exaggerated view of the dignity and position of their favourite group. It is human nature that any subject to the study of which we have devoted ourselves should assume in our eyes larger proportions than it does in the eyes of those who take a wider but less detailed view of it. Hence we see Mr. Butler comparing the Lepidoptera to birds, as if it were a kingdom of equal magnitude, and seeking for

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equivalents for such groups as the hawks and doves within its limits. Whereas it seems to me that the truer parallel is between the whole class Insecta and Birds, and that the equivalent groups for hawks, doves, &c., are to be looked for, not in one of the sections, but in the whole of the class. He looks for both hawks and doves in the *Lepidoptera.* I find nothing but doves. If you want hawks you must go to the dragon-flies, which are their equivalent; and, of course, if we are only dealing with doves, there is nothing in the known phenomena of hybridisation opposed to such a cross having taken place.

It is impossible in the brief space that you would allow me, even to glance at the many arguments that I could adduce to show that this is the true position of the *Lepidoptera.* I hope to do so elsewhere. But I would only remind entomologists, especially lepidopterists, of the trifling characters on which their genera have been established, and how difficult it has been to find any generic characters at all. This is frankly acknowledged as the great difficulty attending the study of *Lepidoptera*, consequently characters which would never for a moment be looked on as generic in any other group of animals, are there allowed that value. If any specialist in another group objects, what is the answer? "We have no better characters, and we must do the best we can with the slight ones we possess." Quite right, in a systematic point of view. If the species of doves came to be reckoned by thousands, the ornithologist would just have to do the same thing; but that would not alter the position of doves in the animal kingdom—they would still bear the same relation that they do now to hawks, and be equally open to hybridisation among themselves, indeed, more so; for such great numbers of one type would be a presumption in favour of every mode by which species could be increased having been resorted to; and this by the way is an additional indirect argument in favour of hybridisation sometimes taking place among *Lepidoptera.*

Of course, I do not mean to say that there is nothing more than specific distinction between the Danaids and Nymphalids. I recognise them as good genera, but only as genera sufficiently nearly akin to allow of hybridisation taking place between them—and *ecce signum*—the mimics in question partaking of the characters of each in all respects as other hybrids do.

ANDREW MURRAY

67, Bedford Gardens, Kensington, Dec. 30, 1870

Measurement of Mass

THE favourite definition of *mass* in the text-books seems to be that the mass of a body is the *quantity of matter* it contains. If we had to do with but one kind of matter this would be intelligible, but I am at a loss to know what is meant when it is said that a piece of cork contains as much matter as a piece of lead. The only satisfactory method of explaining what is meant by the mass of a body, is to define it as a constant belonging to the body, which expresses the proportion between the force (measured statically) acting upon it and the acceleration produced; that every body has such a constant is the result of experiment. The mass of a body has no necessary connection with its weight. We employ weight to measure mass simply because gravity is a convenient constant force. If then we adopt a pound as our unit of *weight*, and use g to denote the force of gravity in reference to a foot and a second as the units of length and time, our unit of mass becomes the mass of g pounds, and this is not variable, although the unit of weight employed is variable; since if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound, precisely in the proportion in which gravity at the Pole is greater than gravity at London.

THE REVIEWER OF EVERETT'S "DESCHANEL"

PHOTOGRAPHIC PROCESSES OF THE PRESENT DAY

THE last two or three years will certainly mark an era in Photography, for not only have several novel and important printing methods been discovered during that period, but other processes of less recent origin have of late been so elaborated and improved as to have become at the present moment practical and easy of manipulation. All of these are, without exception, based upon the action of light upon the bichromates of potash and ammonia; in no single case is the use of a silver salt involved—the agent employed for securing the photographic image in ordinary paper printing—and this is, in truth, a point whose value cannot be too greatly insisted on; for the silver print, be it washed and freed as thoroughly as possible from any deleterious bodies, will always suffer, more or less, from attacks of an impure atmosphere, the delicate metallic film of which the image consists being peculiarly liable to change, from the sulphur compounds and other impurities not unfrequently contained in the air we breathe. And even those silver pictures which do not at first show actual traces of fading or discoloration, will very soon be found, on careful examination, to have parted with some of their original brilliancy, and to lack the pristine freshness which always characterises newly-produced albumenised prints.

It is a great step onwards, then, to have at our disposal practical processes in which the employment of silver may be altogether dispensed with, by the substitution of another material of a more permanent character, either in the form of a chromium compound, or, what is better still, in the shape of gelatinous or greasy ink; and so clear and promising does the photographic horizon appear just now in this direction, as to leave little ground for doubting that before long the practice of printing in silver will be generally abandoned.

All recent printing processes rest, as we have before said, on the action of light upon the bichromates, and here we would parenthetically refer to a simple and familiar experiment which will help very materially to simplify our subsequent remarks. The well-known plan pursued by school-boys for printing fern-leaves and other objects by the aid of the sun, will readily be called to mind by many of us, and this simple manipulation it is that forms the groundwork of the whole series of inventions before us. A sheet of ordinary paper, which has of course been sized, or, in other words, received a thin coating of gelatine, is rubbed over with a solution of bichromate of potash; the latter, as we know, when mixed with any organic body renders the same sensitive to light, and the sizing or gelatine upon the paper becomes in this way endowed with excitable properties. Having been dried in the dark, our sheet of paper is next placed in the sun with the fern-leaf, or other object to be copied, pressed down upon it, and the light acting upon all such portions of the sheet as are not covered up, browns the gelatine there and renders it insoluble; the sizing underneath the leaf, and screened therefore from the light, escapes this reaction and remains soluble, and this, on the printing being completed and the paper washed in water, is at once dissolved away, there remaining a white image of the leaf upon a brown ground composed of bichromated gelatine rendered insoluble by the sun's rays. This experiment may be regarded as the key to the whole question of photographic printing, and by bearing it in mind the reader will have no difficulty in at once comprehending the various inventions of the kind just now being made public.

The first method claiming our attention is the so-called carbon process. Photographic printing of this nature in one form or another has been carried on probably for upwards of fifteen years; but in its experimental stage the mediocre character of the results furnished by it were such as to deprive the system of any material support from photographers, and until, in fact, Mr. J. W. Swan, of Newcastle, made known his method, no easy or reliable *modus operandi* can be said to have existed. The plan followed by Mr. Swan was to prepare a warm solution of gelatine and bichromate of potash mixed with some finely divided pigments, such, for instance, as Indian Ink, and apply this mixture in the form of a coating to a sheet of paper,

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